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ABSTRACT

Previous studies have found that an object can be categorized faster at a basic level (hammer) than at either a subordinate (club hammer) or a superordinate level (tool). While some attribute this result to basic categories having more distinctive attributes, other factors might cause this result. For example, basic categories routinely have shorter and more frequent names than do subordinates and are typically learned earlier and occur more often than either subordinate or superordinate categories. To determine why objects are categorized fastest at the basic level, three experiments were conducted, all of which used artifical subordinate, basic, and superordinate categories, and all of which either held constant or systematically varied several of these "other" factors. All three studies supported the finding that objects can be categorized fastest at the basic level (but the relative speeds of subordinate and superordinate categorizations differed from past results); and all three strongly supported the claim that distinctive attributes are the factor underlying the results, though it appears that only perceptual attributes are critical. (Author/HOD)

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Technical Report No. 254

BASIC-LEVEL SUPERIORITY IN PICTURE CATEGORIZATION

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Abstract

In a seminal paper, E. Rosch, C. B. Mervis, W. D. Gray, D. M. Johnson, and P. Boyes-Braem (Cognitive Psychology, 1976, 8, 382-439) found that an object can be categorized faster at the basic level (e.g., hammer) than at either a subordinate (club hammer) or a superordinate level (tool); they attributed this result to basic categories having more distinctive attributes. But numerous factors other than the number of distinctive attributes might have caused this result; for example, basic categories routinely have shorter and more frequent names than do subordinates, and are typically learned earlier and occur more often than either subordinate or superordinate categories. In this paper, we report three experiments, all of which used artificial subordinate, basic, and superordinate categories, and all of which either held constant or systematically varied several of these "other" factors. All three studies replicated the finding that objects can be categorized fastest at the basic level (but the relative speeds of subordinate and superordinate categorizations differed from past results); and all three strongly supported the claim that distinctive attributes are the factor underlying the results, though it appears that only perceptual attributes are critical.

Basic-Level Superiority in Picture Categorization

When categorizing objects, people must compare perceptual information derived from the object to their stored knowledge of various categories. Typically, the object will match categories at different levels, and two plausible notions lead to opposite predictions about which level of categorization should be easiest. One notion is that it will be easiest to categorize an object into large, abstract categories, since they have few attributes (or features) common to their members. The second notion is that it should be easiest to categorize an object into very specific categories, because more of the object's attributes will find a match. Experiments on picture categorization indicate that both notions are incorrect. Rosch, Mervis, Gray, Johnson and Boyes-Braem (1976) found that people take longer to categorize objects into more specific or more abstract categories than into categories at an intermediate level, which Rosch et al. had previously defined as the basic level of categorization. The purpose of this article is to determine why objects are categorized fastest at the basic level.

Evidence for a Basic-level Superiority

Rosch et al. (1976) operationally defined the basic level as that at which categories are the most

differentiated, i.e., have the maximal number of distinctive attributes. They showed that objects in a basic-level category (e.g., chair) had many attributes in common, whereas members of more general, or superordinate, categories (furniture) had fewer attributes in common. On the other hand, more specific, or subordinate, categories (kitchen chair, reclining chair) had a few more attributes common to their members than did basic categories, but they also tended to have a greater overlap of attributes between categories. (Rosch et al. also claimed that basic categories have a higher cue validity than other categories, but this claim has been disputed by Murphy, in press, so we have not tested it here.)

Because basic categories are more differentiated, Rosch et al. (1976) suggested that objects are first identified as members of those categories. To gain support for this, in one of their experiments (Expt. 7) Rosch et al. presented subjects with a category name and then, one-half second later, with a photograph of an object. The subjects decided whether or not the object in the photograph was in the named category, which was either a subordinate, basic, or superordinate category. Subjects responded fastest for basic categories and slowest for subordinate ones. Rosch et al. suggested that objects are generally identified first as members of basic categories, with superordinate membership then inferred (e.g., if the object is a car, it must also be

a vehicle), and subordinate membership decided by observation of additional features.

While Rosch et al. (1976) performed other experiments (e.g. detection and same-different tasks) that compared performance with subordinate, basic and superordinate categories, these experiments are not as theoretically significant as the picture categorization study we just described. The latter is the only task that Rosch et al. used that directly taps categorization processes--it explicitly requires subjects to categorize objects--rather than, say, to decide whether two objects are the same or different. Also, as Rosch et al. (p. 413) point out, their categorization task is one of the few tasks that shows an advantage of basic over subordinate categories. Picture-categorization tasks employed by other investigators have contrasted only basic and superordinate categories (Brownell, 1978; Smith, Balzano, & Walker, 1978).

Problems with Rosch et als Evidence

There are numerous problems in interpreting the results of Rosch et al's picture-categorization experiment. We will discuss three specific variables that were confounded with the distinction between basic and subordinate categories, any one of which could account for why basic categories were responded to faster than subordinate ones. One variable was name length; subordinate names were two or more words long,

whereas (with one exception) basic names were one word long. With only half a second between hearing the name and seeing the picture, slower comprehension of subordinate names may have been responsible for slower categorization at this level. A second confounding was that subjects may have been totally unfamiliar with some subordinates used (e.g., cross-cutting handsaw, claw hammer), whereas this seems unlikely for the basic categories. The third confounding was that the differentiating features of some subordinates may have been impossible to perceive in the photographs (e.g., green seedless grapes and cling peaches), whereas this was not the case for the basic level. While this last confound is relatively easy to remove, the first two are not when using natural language categories; this suggests the need for artificial categories, which is the tack we will take in the following experiments.

In addition to the above three variables, there were other factors in the Rosch et al. studies that were correlated with the level-of-category factor and that could have caused the effect attributed to levels. These other factors are more general than the "specific confoundings" just discussed; they are more like alternative hypotheses to Rosch et al.'s differentiation explanation of their basic-level superiority. In other words, these factors speak to the question of what makes basic categories "basic."

One alternative hypothesis is that basic categories are superior because they are learned first (see Anglin, 1977). For example, if a child learns the category car some years before vehicle, then s/he may continue to categorize cars as instances of car simply out of "habit." Another hypothesis attributes the basic category advantage to a familiarity factor, reflecting either the frequency of the category, the frequency of its name, or both. Basic category names certainly occur more frequently than subordinate names and there may be a difference between basic and superordinates in frequency as well.[1] There is also another type of frequency that might be used to explain the basic-level superiority--conjoint frequency between a category, or its name, and an object. Even if the total frequency of the words car and vehicle were the same, objects that are in fact cars (Chevrolets, Toyotas, VWs, etc.) may be referred to by the word car more frequently than they are referred to by the word vehicle. Thus, although car and vehicle are equally familiar, the basic name is the preferred label for most cars.

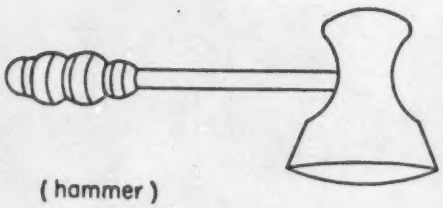
All three of these explanations--order of learning, category frequency, and conjoint frequency--are alternatives to Rosch et al's differentiation hypothesis, but it is difficult to compare them experimentally with natural-language materials. In the following experiments we were able to vary aspects of the category structures and learning procedures to evaluate these hypotheses.

Experiment 1

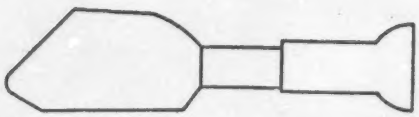
The main purposes of this experiment were: (1) to determine if pictures are categorized fastest at the basic level when the three specific confounds described earlier have been removed; and (2) to evaluate the alternative explanations of the basic-level's superiority.

Fourteen hierarchically-organized categories of novel tools were used. To demonstrate that they resemble natural categories in important ways, we will describe their construction in detail. First, four highly distinctive tools were drawn to be the bases for the four basic categories. If one considers hand tools to consist of a handle, a shaft, and a head, then each of these basic tools was designed to be distinct from the others in each part. An example from each category is in Figure 1. For ease of exposition, they will be called hammer, brick, knife, and pizza cutter (subjects never heard these names).

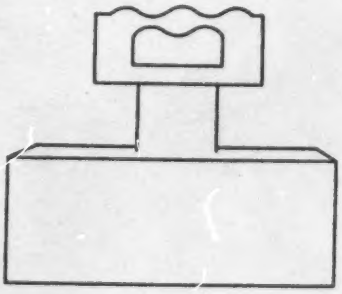
The next step was to construct subordinate categories. Each of the four basic tools was differentiated into two subordinates in the following ways: (1) the hammer had a wide or narrow head; (2) the brick had a single or a two-part handle; (3) the pizza cutter had a long of a short shaft (e.g., the proportion of horizontal to vertical length



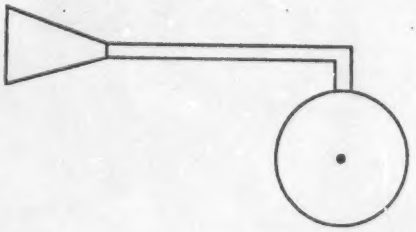
(hammer)



(knife)



(brick)



(pizza cutter)

Figure 1. Examples of the four basic tools used in Experiment 1.

of the shaft varied); and (4) the knife's edge was serrated or straight. To form the superordinates, the hammer and brick were grouped together to produce a category, which for ease of exposition we will call pounders, while the knife and pizza cutter were grouped together to form a superordinate we will call cutters. Figure 2 shows the resulting hierarchical structure of 14 categories (2 superordinates, 4 basics, and 8 subordinates). Associated with each category is a CVC that served as its name in the experiment. Note that below the level of subordinates are actual instances, a total of 16 of them, which were created by making a large and small version of each subordinate item.

Do these categories resemble natural subordinate, basic and superordinate categories? They do, according to the three criteria noted by Rosch et al. (1976): (1) Superordinates have only one attribute common to their members (function), basics have many such attributes (e.g., the general shapes of the head, shaft and handle), and subordinates have only one additional attribute common to their members; (2) The basic level is the highest level at which one can make a unified visual representation of the category--e.g., no mental image could represent all cutters or all pounders; (3) The superordinates are defined functionally (see Rosch et al., p. 392).

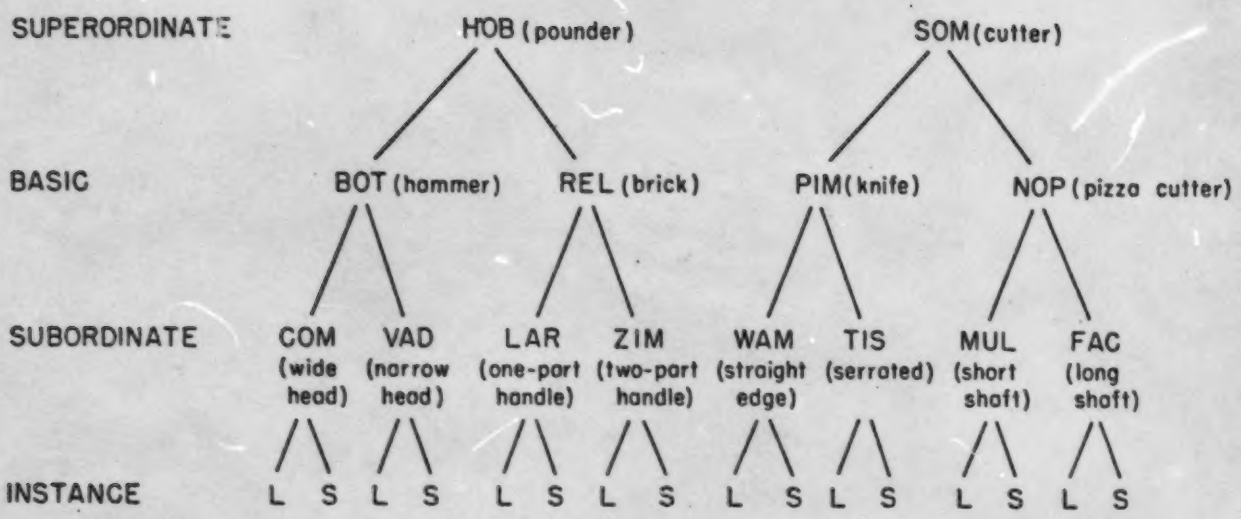


Figure 2. The hierarchy of categories used in Experiment 1. The names in parentheses are for expository purposes only--the CVCs were used as category names in the experiment. The lowest level in the hierarchy denotes the actual pictures used, either a large (L) or small (S) copy of the subordinate tool.

The categories in Figure 2 enabled us to eliminate the three confounds that involved subordinates in the Rosch et al. (1976) study. First, by using CVCs as category names, we insured that all names were equal in length as well as in familiarity. Second, subjects were told the relevant features of all categories, so none was particularly unfamiliar. Third, all differentiating features were perceivable. Other aspects of the design were relevant to the order of learning, category-frequency and conjoint-frequency explanations of the basic-level superiority.

Method

Subjects. Thirty-six Stanford undergraduates participated for pay or to fulfill a course requirement. They were divided evenly into three groups differing in learning order (see below).

Materials. The pictures were of the sixteen specific tools described earlier (see bottom row of Figure 2), photocopied for the learning phase and mounted on slides for the test phase.

The fourteen category names were chosen from Underwood and Schultz's (1960) listing of CVC's with pronounceability ratings below 3.00 on a scale of 1 to 9, where 1 is highly pronounceable. The names used were the syllables from this set with the fourteen lowest meaningfulness ratings on

Archer's (1960) norms.[2] The syllables were randomly assigned to the fourteen categories.

Procedure: Learning Phase. Subjects were tested individually. The basic-first group learned the basic categories first, the subordinates second, and the superordinates last. The subordinate-first group learned the categories in the order: subordinates, superordinates, and basics. The superordinate-first group learned the categories in the order: superordinates, subordinates and basics. If early learning is the cause of basic-level superiority, then whichever category is learned first will be the fastest.

In all three groups, each category was taught with (a) a verbal description, and (b) photocopied pictures of the tools in that category. First, subjects read the description. It gave the category name and a reason for grouping these particular tools in the same category (for subordinates, the reason was the differentiating feature; for superordinates, the reason was the function; and for basic categories, the reason was similar shape). The description also reminded subjects of any previously-learned categories that included tools in the current category; for example, a description of a superordinate category might mention the names of the two basic categories that were its constituents.[3] After reading the description, subjects studied the pictures for as long as they wished.

When all the categories at one level had been presented, subjects were tested on them before learning the next level of categories. The test was an unspeeded version of the categorization task that would be used in the next phase. On each trial, the experimenter said a category name, and then immediately pushed a button, which served to open a shutter on the slide projector after a one second delay. The slide contained a picture of a tool, and subjects indicated whether or not the pictured tool was in the named category by pressing a true or false button: they had been instructed to take as much time as needed in reaching their decision. For each level, there were 2 blocks of 16 trials, during which each picture was presented at least twice, preceded once by the correct and once by an incorrect category name. Feedback was given on each trial. Trials that led to errors were repeated at the end, and if subjects repeated an error or made more than four errors on a block, they were tested on an additional block. Most subjects finished each level in two blocks and none needed more than four.

Though the conjoint-frequency of a category-picture pair was held constant across levels in the learning trials, the overall frequency of a category was greatest for superordinates and least for subordinates. It is impossible to hold constant both conjoint frequency and category frequency if the categories are hierarchically organized,

because the superordinate categories have more members than either of the other levels, and basic categories have more members than subordinate categories. If a category name is presented once with each of its members, the overall frequency must vary. Note, though, that the variation in category frequency favored superordinates, not basics, so the variation should not induce a basic-level superiority.

Categorization Phase. After learning all the levels, each subject was tested individually in a categorization task. The task was the same as that used in the learning phase, except that now: (1) each block of trials contained categories at all three levels; (2) instructions emphasized speed as well as accuracy; and (3) depression of either the true or false button stopped a reaction-time (RT) clock. (Half the subjects in each group used the index fingers of their dominant and nondominant hands to push the true and false buttons, respectively, while the remaining subjects had the reverse assignment.) The experimenter recorded the RT, changed the slide, and immediately started the next trial.

The categorization task had 10 blocks of 28 trials each, with the first two blocks being considered practice. For all blocks, at each category level, half the trials required a true response and half a false response. Over the eight experimental blocks, each category name was

presented equally often on true trials, but with some random variation on false trials (see below). Each picture served in a total of 8 subordinate trials, 4 basic ones, and 2 superordinate ones (with half at each level being true trials). The order of the blocks, and of the trials in each block, was determined by different random permutations for each subject.

The nature of the false trials in a block requires more explanation. For trials with subordinates, the picture and category name could be from either: (1) the same basic category (e.g., the category named the serrated knife, and the picture was of the straight knife); (2) the same superordinate but a different basic category (e.g., the category named the serrated knife and the picture contained a pizza cutter); or (3) a different superordinate (e.g., the category named the serrated knife and the picture was of a brick). These three different trial types offer a variation in how related the category and picture are, and previous work has shown that increases in relatedness lead to increases in false RTs (e.g., Gunther & Klatzky, 1977; Smith et al., 1978). Each block in the present experiment included two trials of Type (1) and three each of Types (2) and (3). (The actual category names used with each picture was chosen randomly, within the constraints of each type.) We also included a variation in relatedness for the false trials with basic names: on two trials the picture was from

the same superordinate as the name, and on two it was from a different superordinate. Finally, for those false trials where the name was at the superordinate level, no variation in relatedness was possible: the target category and picture were always unrelated.

Results

The RTs were averaged and submitted to analyses of variance after discarding any times greater than three seconds. Only correct responses were analyzed, and errors and wild scores were replaced by the mean of that cell for that subject. Error frequencies were too low to allow statistical analysis: the averages were 2.8%, 1.8% and 2.2% for the subordinate-, basic-, and superordinate-first groups, respectively. Discarded scores were less than 1% of the total responses for all three groups.

True RTs. The mean RTs are in Table 1. Separate ANOVAs were performed for each learning group, with level of category, trial-block, and subjects as factors. For each group, basic categories were responded to fastest, followed by subordinates and then superordinates; the overall differences due to levels were reliable, $F(2,22) = 15.23$, $p < .001$, 14.60, $p < .001$ and 3.51, $p < .05$ for the subordinate-, basic-, and superordinate-first groups, respectively. Subjects in the superordinate-first group tended to get faster over blocks, $F(7,77) = 4.10$, $p < .005$,

but this speed-up was not obtained for the other two groups. More importantly, the ordering with respect to levels did not change with practice in any condition, all p's > .20.

Further tests focused on specific comparisons. Within the subordinate- and basic- first groups, basic categorizations were faster than subordinate ones, which in turn were faster than superordinates ones (all comparisons, $p < .04$, by two-tailed sign tests). For the superordinate-first group, basic categorizations were faster than categorizations at the other two levels, $F(1,22) = 5.51$, $p < .05$, but the 44 msec difference between subordinate and superordinate levels was not reliable, $p > .20$.

False RTs. The false RTs in Table 2 are from trials where the category and picture were drawn from different superordinates; by focusing on only unrelated trials, we avoided any confounding between category level and category-picture relatedness (since superordinate false trials were always unrelated, but this was not the case for the other two levels). These false RTs resemble the true data in showing that basic categorizations are faster than superordinate ones; however, subordinates are now slightly faster than basics. In both the subordinate- and basic-first groups, all twelve subjects responded slowest to superordinate categories ($p = .0004$), while the differences between basics and subordinates were nonsignificant by

Table 1

True reaction times (in msec) as a function of category level and order of learning (Experment 1).

Order of Learning	Category Level: Subordinate	Basic	Superordinate	Mean
Subordinate First	737	710	902	785
Basic First	712	655	971	779
Superordinate First	721	670	765	757
Mean	723	678	879	

contrasts, $F(2,22) < 1$ for both groups. The difference due to levels for the superordinate-first group was nonsignificant, $F(2,22) = 1.12$. The effect of blocks, and its interaction with levels, did not approach significance for any group.

To assess the effects of relatedness on false RTs, we conducted separate analyses for trials using subordinate and basic categories. Recall that there were three types of subordinate trials, corresponding to whether the picture and category name came from: (1) the same basic category, (2) the same superordinate, or (3) different superordinates, i.e., were unrelated. As Table 3a shows, subjects responded slowest when the picture and name came from the same basic category. However, when the picture and name were related only by being in the same superordinate, subjects were as fast as they were for unrelated picture-name pairs. The overall effects of relatedness were reliable: $F(2,22) = 23.80, 24.53, 16.48$, for the subordinate-, basic- and superordinate-first groups, all $ps < .001$. All 36 subjects responded more slowly on same-basic than on same-superordinate trials, but the difference between same-superordinate and unrelated trials was non-significant in analyses on the individual groups as well as in an analysis where all the data were combined, all $F's < 1.10$.

Finally, for trials where a basic name occurred, the

Table 2

False reaction times (in msec) as a function of category level and order of learning (Experiment 1).

Order of Learning	Category Level: Subordinate	Basic	Superordinate	Mean
Subordinate First	683[a]	692	903	759
Basic First	668	678	944	763
Superordinate First	722	771	800	764
Mean	691	714	882	

[a]Data are based only on trials containing unrelated category-picture pairs.

picture could either be in the same superordinate as the name or unrelated to it (See Table 3b). The relatedness effects were small and nonsignificant for the basic- and superordinate-first groups, F's < 1, and only marginally significant for the subordinate-first group, $F(1,11) = 3.88$, $p < .10$.

Discussion

The true RTs showed a clear-cut basic-level superiority. Thus the effect is not due solely to variations in name-length and/or perceptibility of distinguishing features, or to the use of totally unfamiliar categories (the three specific confounds in Rosch et al., 1976). Moreover, the basic-level superiority obtained regardless of whether basic categories were learned first or last, and in a situation where the overall frequency of a category and the conjoint frequency of category-picture pairs never favored basic categories. So order-of-learning and frequency factors do not seem to be responsible for the basic-level superiority either. In short, Experiment 1 provides some evidence against a host of alternative accounts of Rosch et al's, results, thereby increasing the plausibility of their distinctive-attributes explanation.

The only notable effect of learning order was that the superordinate-first group responded faster to superordinates than the other groups did. A possible explanation for this

Table 3

False reaction times (in msec) as a function of category -- picture relatedness and order of learning (Experiment 1).

a. Trials with Subordinate Categories:

		Relatedness of Category and Picture			
		Same Basic	Same Superordinate	Unrelated	Mean
Order of Learning	Subordinate First	898	706	683	762
	Basic First	876	684	668	743
	Superordinate First	933	672	722	776
	Mean	902	687	691	

b. Trials with Basic Categories:

Order of Learning	Subordinate First	791	692	742
	Basic First	701	678	690
	Superordinate First	738	771	754
	Mean	743	714	

is that when subjects learned the superordinates first, they could not code them in terms of lower-level categories, which may be the way people typically represent natural language superordinates (Miller & Johnson-Laird, 1976, pp. 281); instead, they may have developed mnemonics to help them categorize the tools directly at the superordinate level, a process that might be more efficient than the natural procedure used by the other groups.

One of our results for true responses, however, is dissimilar from the findings of Rosch et al. (1976); we found that subordinates were processed almost as quickly as basic categories, rather than being the slowest. Thus, Rosch et al.'s results may have been partly determined by variations in factors like name length and perceptibility of distinguishing attributes. The difference between basic and subordinate true RT over our three learning groups was only 45 msec (the difference was significant for two of the groups) so it may be questioned whether there is any real difference between the two levels across all three groups. A four-way analysis of variance (groups x level x blocks x subjects) with only the data from these two levels showed that the 45 msec difference was reliable, $F(1,33) = 15.23$, $p < .001$, and there was no interaction with groups, $F < 1$.

The data for false RTs also showed that basic categorizations were faster than superordinate ones; but now

subordinate categorizations were slightly faster than basic categorizations, by 22 msec over all three groups. In an analysis of variance on these data for all three groups combined, this difference was not significant, $F(1,33) = 1.03$, nor were any of the interactions.

Experiment 2

We have discredited a number of obvious alternative explanations of Rosch et al.'s basic-level superiority, but there is a less obvious one to be considered. Suppose that as a result of learning, subjects constructed a hierarchical representation of the categories, similar to that shown in Figure 2. When a picture is presented, all three category names for that picture are activated to some extent, with activation spreading along the links in the hierarchical network (as in Collins & Loftus, 1975, or Anderson, 1976). Since basics receive activation from both subordinates and superordinates, but the other two levels receive activation from only basics, basics will reach a criterial level of activation sooner. According to this account, then, it is its medial position in a hierarchical structure that causes the basic level to be superior. Experiment 2 tests this alternative by seeking a basic-level superiority with categories that are not organized hierarchically.

Figure 3 shows the categories used. The superordinates do not include the basics, nor do the basics include the

subordinates. (A natural category analogue would be to use furniture and tool as superordinates, apple and orange as basic categories, and collie and dachshund as subordinates.) The basics and subordinates were taken from the previous experiment, while the superordinates were functionally-defined categories composed of new tools. The subordinates have many features common to their members, but also overlap greatly with their contrast categories. The basic categories have almost as many attributes common to their members, but these attributes are distinctive. Superordinates have only the functional attribute common to their members. Thus, although the categories are not hierarchically structured, they meet the same criteria for subordinates, basics, and superordinates as used in Experiment 1.

Because the categories are not organized hierarchically, each category can have the same number of members. Consequently, we can simultaneously hold constant (across levels) both category frequency and the conjoint frequency of a name-picture pair, as well as number of categories at each level (two), category size (two tools per category), and position in the category structure. Another change from the previous study was that the nonsense-syllable names were randomly reassigned to categories.

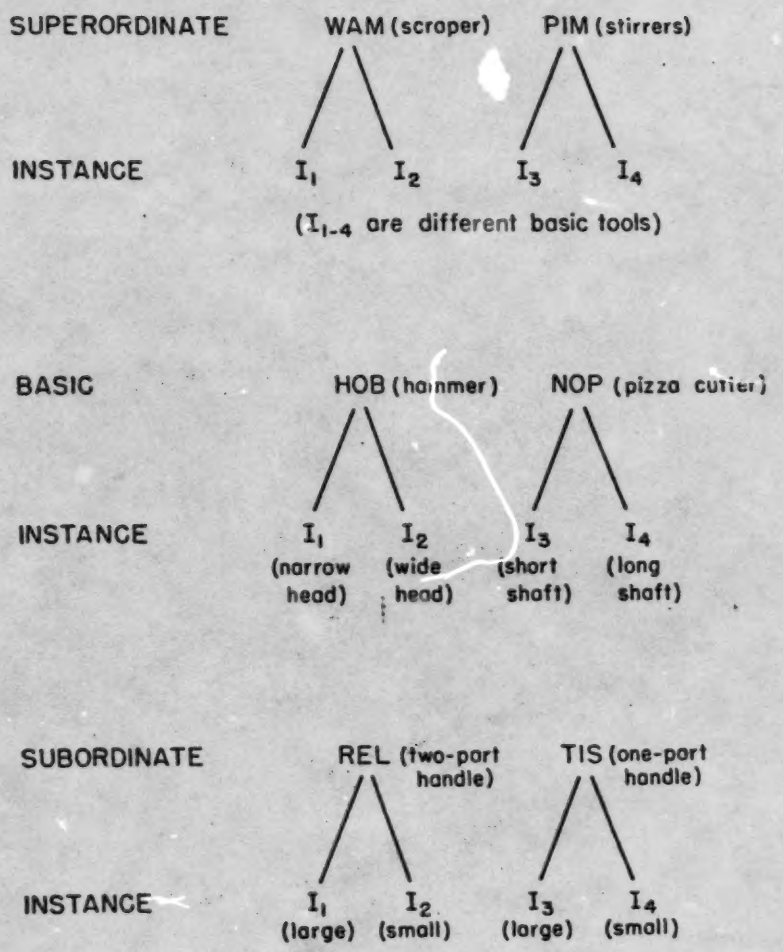


Figure 3. The categories used in Experiment 2. The names in parentheses are for expository purposes only.

Method

Subjects. Sixteen Stanford undergraduates fulfilled a course requirement by performing in the experiment. They were divided equally into two groups differing in learning order.

Materials. The pictures were slides of the twelve specific tools described in the bottom rows of the panels of Figure 3. Each subject learned 2 subordinates, 2 basics, and 2 superordinates. Half the subjects learned one pair of subordinates (the single and double-handled bricks) and half learned another (the narrow- and wide-headed hammers). All subjects learned the pizza cutters and knives as the two basic categories. Two new superordinate categories were created, which we refer to as stirrers and scrapers. The instances of each superordinate were two perceptually dissimilar tools that could be used for the function associated with that category.

Procedure. The learning phase was similar to that of the previous experiment. One difference, however, was that now subjects heard each category name the same number of times (there was no repetition of errors). Another difference was that two new learning orders were used: subordinates- basics- superordinates and superordinates- basics- subordinates.

The categorization task was also similar to Experiment 1: 10 blocks, with the first 2 being practice. However, since now there were only 6 categories, each block had only 12 trials. Each category occurred once on a true trial and once on a false trial in each block, with every picture shown once per block. False trials always contained a category name paired with a picture from the contrast category (i.e., the other category at the same level). Since the different levels of categories were unrelated, none of the relatedness manipulations used in Experiment 1 were possible here.

Results

Analyses of variance were performed on average RTs with errors and wild scores replaced by the mean of that cell for that subject. (A wild score was here defined as a response more than three standard deviations greater than the mean for that subject's cell.) Only 3.9% of the responses were errors, and 1.2% wild scores. The analyses were performed separately for true and false responses, with the factors being levels, order of learning, particular subordinates learned (bricks or hammers), and blocks.

Table 4 contains the results for both true and false RTs. We have collapsed over the two groups that learned different subordinates. (There were no reliable differences between these groups' true RTs, and although the false RTs

showed a 200 msec advantage for the group that learned brick subordinates, $F(1,12) = 5.46$, $p < .05$, there were no reliable interactions involving this factor). The three categorization levels were reliably different, $F(2,24) = 5.03$, $p < .025$ for trues, and $F(2,24) = 6.61$, $p < .01$ falses, the means following the same pattern as in Experiment 1. For true RTs, basics were faster than subordinates, which in turn were faster than superordinates; for false RTs, basics were faster than superordinates and about equal to subordinates.[4]

The true RTs showed no effect of order of learning; its main effect was negligible, $F < 1$, and its interaction with levels was non-significant, $F(2,24) = 2.16$, $p > .10$. For false RTs, again there was no main effect of order, $F(1,12) < 1$, but there was an interaction with levels, $F(2,24) = 6.25$, $p < .01$. As Table 4 shows, when the superordinates were learned first, all three levels were responded to equally quickly, but when subordinates were learned first, basics are responded to fastest, and superordinates by far the slowest. As for practice, although subjects improved significantly over blocks, there was no hint of any interaction involving this factor (all p's > .10 for the true and false analyses).

Discussion

The findings for true RTs replicated the results of the

Table 4

True and false reaction times (in msec) as a function of category level and order of learning (Experiment 2).

Order of Learning		Category Level			
		Subordinate	Basic	Superordinate	Mean
Subordinate First	Trues	805	728	900	811
	Falses	851	829	1085	922
Superordinate First	Trues	926	782	848	852
	Falses	978	982	984	981
	Mean	890	835	954	

first experiment, thereby indicating that the basic-level superiority does not hinge on position in a hierarchical structure, or on category frequency, conjoint frequency, number of categories per level, or number of instances in a category. One difference from Experiment 1, though, is that in the present study true RTs were substantially faster (111 msec) for basics than subordinates, $F(1.24) = 6.98$, $p < .025$. The most likely reason why this difference was so much larger in the present experiment is that subordinate false trials were probably more related than basic false trials--i.e., contrasting categories were more similar at the subordinate level (see Figure 3)--which means that the discrimination between true and false at the subordinate level was more difficult in the present experiment than in the first study.

The present results for false RTs were less clear-cut. When subjects learned categories in the order superordinate-basic-subordinate, the false RTs for the three levels were almost identical; when subjects learned categories in the reverse order, the expected basic-level superiority was found. Why false and true RTs should differ this way is unclear.

For the true RTs, the results for the five learning orders used in Experiments 1 and 2, are unambiguous: basic categories are <u>always</u> fastest, subordinates are usually

faster than superordinates, with the order of the latter two being partly determined by learning order. What explanations are possible for these results? Rosch et al. suggested that basic categories, being the most distinctive, are accessed first, with subordinates identified by inspection of an additional feature, and superordinates identified by inference from the basic level. Although this is a plausible hypothesis, it fails to make predictions for all the comparisons involving false RTs, and it needs to be altered for the non-hierarchical categories used in Experiment 2. For these reasons, and because we desire a more fine-grained analysis of this categorization task, we have been led to different account of the basic-level superiority.

The model we propose is illustrated in Figure 4. On hearing the category name, the subject presumably prepares for the upcoming picture by: (1) activating a perceptual representation of the category (e.g., a visual image or propositional description); and (2) setting criteria on the number of matching and mismatching features that will be needed to trigger true and false responses, respectively. When the picture appears, the subject compares its features to those of the perceptual representation of the category, recording on one counter each match, and on another each mismatch i.e., either a contradiction between picture and category features or a picture feature with no category

counterpart. When the number of matches or mismatches reaches one of the pre-set criteria, the subject responds true or false, respectively.

According to this preparation model, the advantage of basic and subordinate categories over superordinates arises because people typically do not have a single, perceptual representation for a superordinate (as Rosch, et al., 1976, showed); thus people must activate two (or more) perceptual representations when the target category is a superordinate. Maintaining this extra representation during the feature-matching process likely requires extra capacity; and the presence of this extra representation means that extra feature matches will be needed on the average. Both these consequences should eventuate in lengthened RTs compared to the case when the category is at a lower level, and only one perceptual representation is activated.

The advantage of basic over subordinate categories arises because people set differential true and false criteria for categories at the two levels. We assume that criteria are set so as to maximize discrimination between the target category and any category that contrasts with it. For example, if the category presented was brick, criteria are set so as to maximize discrimination from hammer, since that is the closest "false" category. Since subordinates have greater overlap with their contrast categories than do

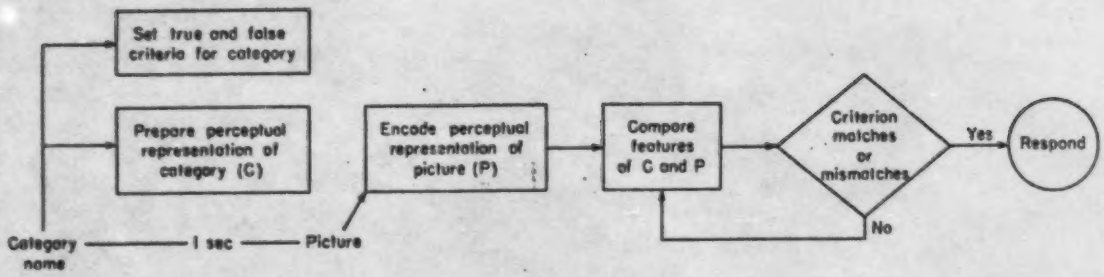


Figure 4. The preparation model for the categorization task.

basics, the true criterion should be set higher for subordinates. And the higher the criterion, the longer the feature-comparison process, and the longer the RT. To illustrate, if the category is wide-headed hammers (a subordinate) but the narrow-headed hammer is shown, there will be many matches output from the comparer so a high criterion for true responses is needed to avoid false alarms. However, if the category is hammer (a basic), no other object besides a hammer will produce many matches, so fewer matches are required to reach a true decision.

For false responses, however, the criterion should be set slightly lower for subordinates than for basics. The rationale for this hinges on the fact that a mismatch can mean either that a picture feature contradicted its counterpart in the category representation, or that a picture feature found no counterpart in the category representation. The occurrence of a contradiction is equally diagnostic of a false response for subordinates and basics; failure to find a counterpart, however, is more diagnostic of a false response for subordinate-category representations than for basic ones because the former contain more features. This line of reasoning suggests that false RT should be slightly faster for subordinates than basics, which was the case in Experiment 1 though not in Experiment 2. This prediction, though, is too strong. It ignores differences between subordinates and basics that do

not have to do with the decision component (e.g., aspects of the feature-comparison process). A more conservative prediction is that the advantage of basics over subordinates should be greater with true than false responses. This prediction obtained in both the present experiments, as well as in Rosch et al (1976).

Finally, the relatedness effects on false RTs would be attributed to the feature-comparison process of the model. When the picture and category representation are perceptually similar, false responses will take longer since the comparator will take longer to find the criterion number of mismatches. This predicts that for trials with subordinate categories, false RTs should be long when pictures and categories were related at the basic level but not when they were related only at the superordinate level, because only the former are perceptually similar. Identical reasoning predicts that for trials with basic categories, false RTs should be the same for pairs related at the superordinate level and for unrelated pairs. Both these findings were obtained in Experiment 1 (they could not be tested in Experiment 2).

The preparation model ignores naming and inferences, but this may not be unreasonable for well-practiced subjects with a restricted set of materials (conditions that also characterized Rosch et al.'s Experiment 7 and Smith et al's experiments).

Experiment 3

This experiment tests two claims of the preparation model: (1) true RTs to superordinates are slow because such categories lack a unique perceptual representation; and (2) the relatedness effects on false RTs are due to perceptual not semantic similarity.

To get at these issues we used categories that had the same hierarchical structure as that in Experiment 1: two high-, four middle- and eight low-level categories. Unlike the earlier experiment, though, the present categories did not fall into the usual superordinate-basic-subordinate order--see Figure 5. The eight low-level categories were basic (four basics from Experiment 1 plus four others); the four middle-level categories were defined functionally (tools for cutting, pounding, scraping and stirring); and the two high-level categories were defined perceptually (large and small tools).

Based on the preparation model, we expect the following: (1) since each low-level category has many distinctive perceptual attributes, subjects should be able to prepare a unitary perceptual representation of it and rapidly categorize the picture; (2) since each middle-level category has no obvious perceptual feature (like most

superordinates), subjects should be unable to prepare a perceptual representation of it, and hence their categorization times should be substantially longer than for the low-level categories; and (3) since each high-level category has a distinctive perceptual feature, namely its size, subjects should be able to prepare a perceptual representation of the category (prepare to see a large or small figure), and their categorization times should be faster than those associated with the middle level.[5] Thus we expect categorization times to first increase and then decrease as we move up the hierarchy, the exact opposite of the pattern we obtained in Experiment 1.

These categories also allow us to test our claim that relatedness effects on false RT are due to the perceptual similarity of the category and picture, rather than to semantic similarity (which, in these experiments, means membership in the same category). In Experiment 1, the major relatedness effect was that, on subordinate trials, RTs were slower to name-picture pairs related at the middle-level (basic) than to pairs related at only the high-level (superordinate). Presumably this effect came about because representations of pairs related at the middle level shared more perceptual features than representations of pairs related at only the high level; however, these pairs were also more semantically related than those related at the high level. In the present experiment, name-picture pairs

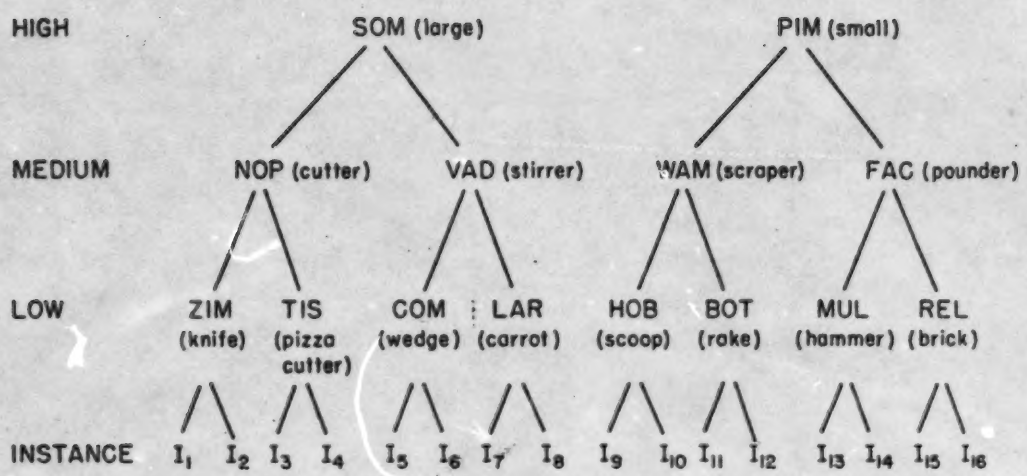


Figure 5. The hierarchy of categories used in Experiment 3. The names in parentheses are for expository purposes only.

related at the middle-level (i.e., with a common function) should not take longer than pairs related at the high-level because the representations of the former are no more perceptually similar than those of the latter. Experiment 1 essentially serves as a control for this null prediction because the semantic structure is identical for both studies.

Method

Subjects. Twelve Stanford undergraduates fulfilled a course requirement by participating in the experiment. They were divided into two groups based on a post-experimental questionnaire (described below).

Materials. The 16 pictures included 8 used in Experiment 1, plus 8 additional ones. The latter included two variations on the four basic tools used in Experiment 2, and were constructed with the same constraints as used in Experiment 1. That is, each category at the lowest level contained two tools that were highly similar to one other (e.g., serrated and straight-edge knives) but differed greatly from other tools at that level. Pairs of these low-level categories were combined according to function to form four middle-level categories--stirrers, cutters, scrapers, and pounders. Then pictures of the stirrers and cutters were enlarged and combined into one high-level category, defined by the feature of "large", while pictures of the

scrapers and pounders were reduced and formed the other high-level category, that defined by "small". The small pictures were at most 33% of the area of the large pictures. The category names were the same as those used in the previous experiments, but were randomly re-assigned to categories.

Procedure. The learning procedure was the same as that of Experiment 1, with appropriate changes in the category descriptions for the new functionally-defined and size-defined categories. All subjects learned the middle categories first, the low-level categories second and the high-level categories last (following the basic-first condition of Experiment 1, although here the middle-level categories are not basic).

The categorization procedure was the same as Experiment 1, with the addition of a questionnaire at the end of the testing. One question asked subjects what they did in the interval between name and picture, e.g., "keep your mind blank?", "try to think what the named tool would look like?". Another question asked subjects what they did when the experimenter said the names of the high-level categories (those defined by size), e.g., "try to think of all pictures with that name? try to think of one particular picture with that name? get ready to see a large or small picture?" As explained in the Results section, the last question was used to divide the subjects into two groups.

Results

In pilot testing, some subjects reported that they ignored the size cue. The questionnaire used in the main experiment allowed us to divide the subjects into those who used the size cue and those who did not: eight subjects chose the response "get ready to see a large or a small picture" to the question of how they prepared for the high-level categories (these subjects comprise the size group), while four chose a different response (the no-size group). Our predictions about the effect of category levels on RT apply only to the size group. Although there are only four subjects in the no-size group, the differences between the groups are striking enough to be of interest. There were an average of 2.2% and 1.9% errors, and of 0.7% and .3% wild scores for the size and no-size groups, respectively. Again, these were too few errors to analyze meaningfully.

True Rts. The average true RTs are in Table 5. For the size group, the results are as predicted: the low-level (basic) categories were fastest, the middle-level (functionally-defined) categories were slowest, and the high-level (size) categories were inbetween. The overall difference due to category level was highly reliable, $F(2,14) = 22.7$, $p < .001$. And orthogonal contrasts showed that the middle categories were slower than the other two, $F(1,14) = 41.6$, $p < .001$, and that the superiority of the

low over the high-level categories was marginally significant, $F(1,14) = 3.85$, $p < .10$. For the no-size group, the data followed the pattern expected if middle- and high-level categories were inferred from low-level (basic) categories; RT increased with level in the hierarchy, $F(2,6) = 13.1$, $p < .01$.

False RTs. As usual, only unrelated category-picture pairs were analyzed to test the effect of categorization level. For the size group, false RTs followed the pattern of the true data (see Table 5), $F(2,14) = 8.5$, $p < .005$. Orthogonal contrasts again showed that the functionally-defined, middle-level was slower than the other two, $F(1,14) = 10.4$, $p < .01$, and that low-level categories were responded to faster than high-level ones, $F(1,14) = 6.5$, $p < .025$. The pattern for the no-size group also followed the true data: RT increased with hierarchical level, $F(2,6) = 13.9$, $p < .01$.

Relatedness effects were first examined for trials where the category was from the low level. The means are in Table 6. The overall effect of relatedness was substantial in the no-size group, $F(2,6) = 7.9$, $P < .025$; however, it failed to reach significance in the size group, $F(2,14) = 2.2$, $P > .10$. The latter null result is surprising; while we did not expect any difference between name-picture pairs related at the middle-level and those related only at the

high-level (for both kinds of pairs, only the feature of size is shared), we did expect RTs to be fastest to unrelated pairs (they contain no shared perceptual feature). In any event, the same expectations apply to the no-size group (there is no reason to expect any difference between the groups on subordinate trials), and here, the pattern of results is as predicted; there was no difference between name-picture pairs related at the middle level and those related only at the high level, $F(1,6) < 1$, while RTs were faster to unrelated pairs than to those related at the high level, $F(1,6) = 14.8$, $p < .05$.

Trials where the category names were from the middle level offer us another chance to check the relatedness predictions. Again we expect faster RTs to unrelated name-picture pairs (no shared, perceptual feature) than to pairs related at the high level (size feature shared). This time the data are completely in line with expectations--See Table 6. In the size group, RTs were about 200 msec faster to unrelated pairs than to pairs related at the high level, $F(1,7) = 6.3$, $p < .05$, and the no-size group showed a comparable trend, $F(1,3) = 7.3$, $p < .10$. These results contrast strongly with those of Experiment 1, where relatedness at the high level (i.e., being members of the same superordinate) never produced a significant effect, even though each group there had more subjects than either group here. This contrast in results between the two

Table 6

False reaction times (in msec) as a function of category picture relatedness and group (Experiment 3).

a. Trials with Low-Level Categories:

		Relatedness of Category and Picture			
		Same Middle Level	Same High Level	Unrelated	Mean
	Size	669	617	600	629
Group	No-Size	794	768	689	750
	Mean	732	692	644	

b. Trials with Middle-Level Categories:

	Size	1025	824	924
Group	No-Size	1134	840	987
	Mean	1080	832	

experiment attests to the perceptual nature of relatedness effects, since the highest level was defined perceptually here but functionally in Experiment 1.

Questionnaire responses. All subjects but one said that while waiting for the picture to appear, they tried to think what the named tool(s) would look like and prepared to see a picture like that. These self-report data lend support to a critical assumption of the preparation model.

Discussion

The results generally supported the preparation model. True RTs for the size group were faster for categories defined perceptually than for those defined functionally, and analyses of relatedness effects on false RTs generally indicated that perceptual similarity between target category and picture, rather than semantic similarity, determines the ease of responding. (The one exception to this being the null results for the size group on trials with subordinate categories). These conclusions rest partly on comparisons with Experiment 1. The hierarchical structure of the categories there was the same as in the present study, but there the middle level was the fastest while here it was the slowest. Also, in Experiment 1, relatedness at the high level had no effect on false RT while in the present study it did. Furthermore, all 36 subjects of Experiment 1 had slower RTs when picture and category are related at the

middle level (there, the basic level), but no such effect was found here (where the middle level was functionally-defined). Given these contrasting results, it seems most unlikely that the results in either experiment were in any way an artifact of the particular names used, number of categories at each level, category size, etc.

General Discussion

The Role of Mediation

One criticism of this research is that the categories used may not have been "artificial" enough, i.e., subjects might have associated each of our categories with the natural category most similar to it and used the latter as a mediator. If our basic categories were mediated by natural basic categories, then many of our results could be artifactual. (This hypothesis could not explain all our results, e.g., why the size categories of Experiment 3 were faster than the function categories.) To evaluate this hypothesis, we asked 13 Stanford students to name a picture from each basic category used in Experiment 3 (which included those used in Experiments 1 and 2) in order to determine the similarity of our pictured tools to natural objects. For comparison, we included an equal number of

line drawings of real objects. After naming each picture, subjects rated how well the picture fit that name on a 7-point scale (where 1 meant the picture fit the name very well, and 7 meant that they were only guessing what the name was).

Subjects found only poor names for the artificial categories, with one exception. Seven of the pictures received median ratings of between 4 and 6 on the scale, seemingly indicating that the pictures were not good examples of natural categories. Furthermore, the suggested names were often vague (e.g., "design") or were the names of part of an object (e.g., "front of farm equipment")--labels that would not be efficient mediators of perceptual categorization. In contrast, all the pictures of natural objects had median ratings of 1.5 or lower, indicating that these objects were recognizable members of natural categories (though not always basic categories). The one exception to the results for artificial categories was the picture of the hammer; its median rating was 1. However, the most popular name for this picture was not "hammer," but rather "ax" or "hatchet" (given by 9 out of 13 subjects), which seems to be an inappropriate mediator for a tool known to be a pounder. Furthermore, the basic category advantage was found in Experiment 2, both when the hammer pictures did not occur at all, and when they occurred only as subordinate categories, suggesting that natural category mediation was not a factor in these experiments.

General Implications

Rosch et al. (1976) claimed that objects are categorized faster at the basic level than at the subordinate or superordinate levels because basic categories are associated with more distinctive attributes. Our studies provide strong support for this claim as long as it is qualified to mean perceptual attributes. But, though we agree with Rosch et al. on the importance of distinctive attributes, our preparation model differs appreciably from their rough view of processing. Their view must assume that objects are categorized first at the basic level, and further assumes that categorization at the subordinate or superordinate level requires an additional process (either checking a distinguishing feature, or drawing an inference). In contrast, the preparation model in no way assumes basic categories are special. Rather, the superiority of basic categories is a natural consequence of the model's processing assumptions and the fact that basic categories have more distinctive features.

The preparation model is also compatible with other findings on picture categorization. Consider Smith et al's (1978) finding that typical objects are easier to categorize than atypical ones, (e.g., a pictured robin is categorized as a bird faster than a pictured chicken is). The preparation model can handle this result if it is assumed

that typical instances are likely to share more perceptual features with the category than do atypical instances (see Smith, Shoben, & Rips, 1974). Another finding of Smith et al. (1978), however, seems to conflict with the preparation model. Specifically, they found that false RTs were slowed when the category and picture were related at the superordinate level, where birds, fruit and vegetables comprised one superordinate (living things), and tools, furniture and clothing comprised the other (non-living things). The preparation model has difficulty explaining why it should be harder to respond that a pictured chair is not a tool than that it is not a vegetable, since the picture should have minimal perceptual overlap with either category. It seemed to us, however, that fruit and vegetables share many perceptual attributes, while the other categories do not. Therefore, we reanalyzed the Smith et al. false data, eliminating fruit-vegetable pairs. Before reanalysis, the relatedness effect in question was a significant 52 msec; after elimination, it dropped to an nonsignificant 14 msec (the fruit-vegetable pairs took 220 msec longer than the mean of the other "related" pairs).[6] This post hoc analysis thus provides further evidence for the preparation model.

What is the applicability of the preparation model to categorization in everyday activities? The model presupposes that a target category is pre-specified, as when

a person tries to identify objects to achieve some goal (e.g., looking for chairs vs. furniture in a cluttered department store). Obviously, people can categorize objects even when the category is not specified beforehand--in fact, this may be the more typical situation. (In such cases, the model described by Brownell, 1978, in which an object activates the category that matches it most closely, may be most appropriate.) Perhaps specifying a model with such a restricted range of applicability seems a trivial endeavor to some. We do it, however, out of the conviction that it is impossible to specify and empirically test category representations without a fairly detailed hypothesis of what processes act on them to produce the obtained results (see Smith & Medin, 1981, chapter 3 for a more detailed argument).

As a way of attesting to the model's utility, let us highlight what it tells us about category representation by listing some forms of representation that are incompatible with the model. First, the representation is not one where certain categories are tagged as "basic" (as a result of frequency or early learning, or whatever) and then are preferentially used. Experiments 1 and 2 argued convincingly against this. Second, the basic category advantage cannot be a side-effect of conversational pragmatics (see Cruse, 1976, for an analysis of this sort). For example, one might suggest that the Grice's (1975) maxim

"to be informative but not more so than required," entails that people prefer to use basic categories. However, it is difficult to conceptualize "informativeness" such that it does not presuppose the theory of basic categories that Rosch et al. proposed, yet still predicts our results. For example, if informativeness depends on how many different objects a category denotes, as well as how many it distinguishes, then the category structures in Experiments 1 and 3 should have led to identical results--yet they showed many striking differences. Third, the basic category is not merely the middle category of a hierarchy (or, the middle of the commonly-used categories of a hierarchy). Experiments 2 and 3 gave strong evidence contradicting this hypothesis.

Furthermore, our model's focus on perceptual attributes ties in with a recent analysis of Rosch et al.'s proposed category structure by Hemenway (1981). She found that the basic level was the highest level judged to have many parts common to category members, but that this was not true of other types of attributes e.g., qualities and functions. This would explain the importance of perceptual features, since these are closely related to parts of objects (as opposed to functions, which may have many different perceptual instantiations). It may be that natural categorization has evolved to attend primarily to perceptual attributes, and that functional attributes are always translated into disjunctive perceptual identification

procedures for categorization purposes (see Miller & Johnson-Laird, 1976, and Smith & Medin, 1981, for detailed discussions of these issues).

So, even if our proposed processes are limited to cases where a target category is pre-specified, our proposed representations may be quite general. That is, there is no reason to believe that the claims we have made about category representations--e.g., that basic categories have many distinctive perceptual properties--are task-specific. Also, the model is easily extendible to other questions of interest in categorization. For example, Rosch, et al. (1976) have noted that experts may have different basic levels (in their area of expertise) than the rest of the population, presumably because they know more about subordinate categories. Our model would predict that as one learns more distinctive perceptual features at the subordinate level, one's subordinate categorizations should become more rapid, perhaps eventually being faster than categorizations at what is normally the basic level. Moreover, people who don't even know of the existence of many subordinate categories (e.g., small children) may also categorize faster at this level, since they know fewer overlapping subordinate categories than most people. In this case, the effect would be due to a low criterion for true decisions for the subordinate category, causing these people to respond faster, but also to make many errors at this level.

Footnotes

[1] Such a variation in familiarity needs to be distinguished from the specific confounding noted before that some subordinates may be totally unfamiliar. Earlier we were concerned with a binary distinction between categories that subjects had some knowledge of versus those that were totally unfamiliar; now we are concerned with a continuous variation in familiarity among categories, most of which subjects have some knowledge of.

[2] One syllable was discarded because it was too similar to an already chosen one; the next least meaningful syllable on the list replaced it.

[3] Through an oversight, the Basic-first group's descriptions for the superordinates did not mention the subordinates even though the latter had already been learned. This seems unimportant, since a group very similar to this one (see Footnote 5), but without the error, gave the identical pattern of results as this group.

[4] We should point out that the effect of levels is partly confounded with materials, i.e., different pictures were used at the three levels of categories. We attempted to minimize this confounding by using two sets of subordinates, and both showed the basic-subordinate difference. But this does not rule out the chance that our

levels effect is partly a picture effect in disguise. However, this possibility seems to be ruled out by the results of the next experiment, since there each of the present pictures was tested at all levels of categorization, and we found a levels effect like that of the present experiment.

[5] We know from other work that not all perceptual features are sufficient for preparing a representation. In a study not reported here, we used the same categories and materials as in Experiment 1, and defined the superordinates not by function but by a perceptual feature that described the texture of the handle. What we have called cutters were defined by having plain handles, while pounders were defined by having textured handles (see Figure 1). Subjects seemed unable to prepare a perceptual representation of a category given only this perceptual feature, and the results were virtually identical to those of Experiment 1; i.e., categorization was slowest at the highest level. After the fact, these results seem unsurprising; a glance back at the tools in Figure 1 indicates that the handles are not at the same locations for all tools, and the hammer's textured handle is quite different from the brick's. These kind of problems do not seem to arise when the feature is a particular size value.

[6] Rather than demonstrating an effect of perceptual similarity, perhaps this change in results simply indicates that Smith et al's pictures of fruits and vegetables were not as good as the other pictures, so subjects took longer to categorize them. This explanation fails, though, as other judgments using the fruit and vegetable pictures were 59 msec faster than judgments involving the remaining pictures.

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